Musician playing wind instruments and risk of lung cancer: is there an association?

Lung cancer is an important public health problem. Tobacco is its main risk factor. Occupation is also an important risk factor. Some jobs have shown higher risks than others, but few investigations have asked about activities or hobbies in leisure time in relation to the risk of lung cancer. A case-control study was performed between 1999 and 2000 in the Santiago de Compostela Health District (Galicia, north-west Spain). A total of 132 cases with confirmed diagnosis of lung cancer and 187 controls were enrolled. Controls underwent trivial surgery at the same hospital as did the cases. A personal interview about lifestyle and activities (past and present) was conducted by a trained researcher. We found that, besides tobacco and occupational exposure to carcinogens, some leisure time activities were risk factors for lung cancer. Among the cases there were two musicians who played wind instruments, whereas there were no wind instrument players among the controls. The two cases had been playing the clarinet and trombone for 35 and 30 years respectively. Both were ex-smokers (moderate smokers) and played music as a hobby. They had epidermoid lung cancer and were diagnosed at 57 and 76 years of age.

Since in our population the prevalence of persons playing musical instruments and specifically wind instruments is extremely low, we think that this activity might be a risk factor in development of lung cancer. The very low number of persons playing this type of musical instrument is probably a reason for the lack of studies focused on this activity, as many occupational studies of lung cancer and occupation are based on registries of workers. One study found an increased mortality rate of lung cancer for a category that included painters, potters, musicians, and actors—an inhomogeneous category that did not allow us to extrapolate results. The results were not adjusted according to smoking history.

This hobby requires inspiration and breathing of large volumes of air, making the lung alveoli expand more than in other people. This fact could facilitate the penetration of carcinogens in the cells of the lung epithelium, and this could be more harmful in smokers. We have found no other studies that have reported this possible association. It would therefore be necessary to explore this association in greater samples of professionally exposed persons in order to determine whether this finding is consistent or due to chance.

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References

The paper by Harrison and colleagues and the accompanying editorial by Cherrie in the October 2002 issue of Occupational and Environmental Medicine address the important issue of personal exposure assessment (of air pollutants) in environmental epidemiology. After reading both papers we would like to make a few comments with regard to the design, conduct and statistical analysis of the study by Harrison et al and at the same time answer the question raised by Cherrie in his editorial.

Coming from the occupational exposure assessment arena it is interesting to see that our environmental colleagues are still relying on a large-to-medium scale (environmental) sampling and even rely on shadowing to represent personal exposure. The latter brought back memories of old occupational hygiene textbooks with pictures of technicians standing with a sampling probe in the breathing zone of a worker (clearly hindered while carrying out his work task). It is interesting to note that Dr Cherrie’s very relevant earlier work on whether wearing sampling pumps affects exposure (it hardly did) was not mentioned in both papers.

The paper by Harrison and colleagues clearly states as one of its goals to answer the question “Does modelling through the use of microenvironment measurements and activity diaries produce reliable estimates of personal exposure to air pollutants?”. However, in the only setting where personal exposures were actually measured (phase 1, volunteers; with regard to phase 2 we do not think that shadowing results can be seen as equivalent to personally measured exposure) it is hard to grasp from both fig 1 and table 2 which exposure was actually modelled (1 hour averages, 2-3 day averages) and how a formula was only provided for measurements within the susceptible groups.

When comparing direct personal measurements for CO and PM10 with the modelled results the authors exclude all data that resulted in a not directly comparable—that is, when the volunteer spent most of their time out of house, and all the data for smokers. It is therefore not surprising that good correlations were found between personal and static measurement results. Why were smokers excluded? Was their measured CO exposure representing a different kind of CO leading to a different health effect? We know that excluding smokers or people with unventilated gas heaters is common practice in the statistical analyses of environmental exposures, but this would only make sense if we were expecting different risks from the same microenvironment originating from different sources.

In fig 1 the authors present 120 comparable data points for 11 individuals; given the repeated nature of the sampling these data points cannot be seen as statistically independent. Putting a simple regression line through these points is therefore not correct and application of a mixed effects model would have been more appropriate. Besides that, when estimating environmental exposure, for instance, for a panel study, we are interested in the full range of exposures both in the temporal and spatial sense (not only for the room with the static sampler). However, Harrison et al conclude, “...modelled personal exposure is unable to reflect the variability of measured personal exposures occasioned by the spread of concentrations within given microenvironments”.

Both Cherrie and Harrison et al claim that microenvironmental sampling would be a good alternative for direct personal exposure measurements that supposedly are “costly, time consuming”. However, the costs for sampling microenvironments in a general population study will be far greater if we want to measure all the microenvironments people end up in (for instance, in table 1 seven environments are indicated, and most of them will most likely be different for each study participant). In addition, it will be practically impossible to measure some of these environments as the authors point out. In their study, it was not possible to collect data for all appropriate microenvironments, even for a comparatively small number of subjects.

Recently, a very insightful paper was presented at the X2001 conference in Gothenburg. Seixas and colleagues showed that in a study to assess occupational noise exposure, a task based methodology (analogous to microenvironmental sampling in environmental exposure assessment) could only account for 30% of variability in daily exposures. They even considered this estimate somewhat optimistic since their estimated noise exposures were derived from the same data on which the daily average exposures were estimated. In addition they clearly pointed out that using simple task based averages that artificially compress exposure variability resulted in a very substantial negative bias in the estimated daily exposure.

In our opinion, we should aim to collect personal exposure measurements when estimating exposure for epidemiological studies.
We agree that smaller and lighter sampling instruments will need to be developed, as suggested by Cherrie in his editorial. Recent studies in both the occupational and environmental arenas have shown that study subjects are capable of carrying out personal measurements, and that exposure assessment is feasible within this group. 12 Endotoxin exposure to some of these workers appears to be sufficient to induce a respiratory response characteristically associated with endotoxin. However, there appears to be a large discrepancy in endotoxin exposure assessment and grouping of occupational exposures experienced in the general environment. Recent modelling based on diaries will not provide sufficient resolution and accuracy, given the relatively low concentrations in the general environment, we will need to measure these accurately. Microenvironmental monitoring and consequent modelling based on diaries will not provide sufficient resolution and accuracy.

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References

Will sewage workers with endotoxin related symptoms have the benefit of reduced lung cancer?

Thorn and colleagues' reported that sewage workers suffer from various symptoms which can be related to bacterial endotoxin (lipopolysaccharide) exposure. Other studies12 have shown that some members of this occupational group are commonly exposed to endotoxin. However, there appears to be a large discrepancy in endotoxin exposure assessment and grouping of occupational exposures experienced in the general environment. Recent modelling based on diaries will not provide sufficient resolution and accuracy, given the relatively low concentrations in the general environment, we will need to measure these accurately. Microenvironmental monitoring and consequent modelling based on diaries will not provide sufficient resolution and accuracy.

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Neurobehavioural testing in workers occupationally exposed to lead

The article of Dr Goodman and coworkers on “Neurobehavioural testing in workers occupationally exposed to lead” covers an interesting approach with a surprising main message: “None of the individual studies is conclusive regarding meta-analyses of the neurobehavioural effects due to lead.” However, the repeated information on cross sectional studies should also be accepted as source for conclusions on (neurobehavioural) effects due to exposures. Meta-analyses are one approach to search such summarising information. Changing into account that the extended study selection in the article of Goodman et al may lead to different results we do not agree with several arguments. For example, they refer to the bias problem, the exposure range, the interpretation in terms of age related changes, and the results for the digit symbol test. On these problems an exchange of opinions has been published in Archives of Toxicology.1 Without making reference to this discussion, several arguments and conclusions were presented again. They are identical with the main conclusions in an anonymous “expert opinion” for the German Battery Association.2

From our point of view it makes no sense to repeat the same detailed argumentsation for the second time. However, we believe that the readers of your journal should be informed that the conclusions of the article of Goodman et al have been discussed in other places. In the meantime an additional article on the subject has been published.3 In this article the data of the original “expert opinion”—the basis of the article in Occupational and Environmental Medicine—and the data of our first meta-analyses were comparatively evaluated. We hope that the critical readers of your journal pick up the full information on the matter. Thereupon they may draw their own conclusions regarding meta-analyses of neurobehavioural effects due to occupational exposure to inorganic lead.

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Authors’ reply

We thank Drs Seeber and Meyer-Baron for their comments on our paper,4 and also Drs Schwartz, Stewart, and Hu for comments published in the September 2002 issue of OEM.5

The following is our response to the specific criticisms made by Schwartz and colleagues: (1) “No evaluation of the quality of the evidence from available studies based on study design and analytical method.” Study quality assessment was the first task we completed. As discussed in our methods section, our quality criteria included evaluating pre-exposure status, use of blinded procedures, and adjustments for age, other occupational exposures, alcohol and tobacco, endocrinological factors (income level, education, etc.).

(2) “Data were combined from poorly done studies with data from well done studies.” Table 1 shows that no study satisfied all of the above quality criteria. Schwartz et al did not provide criteria to distinguish a “poorly done” from a “well done” study. However, we conducted an additional analysis of the five relatively well designed studies that adjusted for age, education, and alcohol use (Baker and colleagues,6 Campara and colleagues,7 Chia and colleagues,8 Maidish and colleagues,9 and Williamson and Teo). These five allowed us to conduct a meta-analysis for only three tests. For the Santa Ana preferred hand test, the effect size changed marginally from non-significant negative to non-significant positive. For the Santa Ana non-preferred hand the result changed slightly towards the null and remained statistically non-significant. For the digit symbol test, the result remained positive from the null and remained statistically significant in the fixed effects model, but changed slightly towards the null and was no longer statistically significant in the two random effects models.

(3) “Inclusion of studies that did not control for age and education.” Schwartz et al do not provide evidence that age and education are the two most important predictors of lead neurobehavioral effects. We argue that alcohol use or the presence of pre-existing neuropsychiatric conditions could also act as powerful confounders. The studies in our meta-analysis had different strengths and limitations and further inclusion or exclusion based on quality would be a matter of judgement. However, an additional analysis based on the 13 studies that adjusted for age and education was conducted. We oppose to our original findings based on all 22 studies, none of the tests showed a statistically significant difference in all three models. (See OEM website for results table.)

(4) “No adjustment for age, sex, or lead dose across studies.” This criticism appears to be somewhat theoretical, as the data did not allow such adjustment.

(5) “Reliance on exposed vs. non-exposed comparisons” rather than “only including studies that reported beta coefficients for the blood lead versus test score relation, or adjusting for mean blood lead levels in exposed and non-exposed groups.” We used the same definition of exposure as the previously published meta-analysis by Meyer-Baron and Seeber7 (less than 70 µg/dl) to find out if the results of our two studies were reproducible. The direct comparison of the two analyses in the discussion is, therefore, important in explaining our position with regards to meta-analysis as a research technique. We agree that other approaches could also be informative. The statement “The authors conclude that the blood lead levels, that are described as ‘moderate’ in one location in the manuscript and ‘low’ in another, are not associated with neurobehavioural test scores” misrepresents our conclusions listed on page 222 of our paper.

(6) “Reliance on a small number of unspecfied studies for effect estimates. Table 2 of the study reports the number of studies that were combined to derive effect estimates, but does not specify which studies were combined.” The original manuscript of the paper included information on each individual study; however, based on the reviewers’ and editor’s comments, we had to shorten the manuscript substantially. We will make this information available on request.

1 Schwartz et al. Met al 2001 article by Schwartz and colleagues; this article was unavailable when our manuscript was submitted for publication in December 2000. The other two studies they cite did not meet our inclusion criteria. While we have not had an opportunity to evaluate the association between cumulative exposure to lead and neurobehavioural testing results, we did note that the
We have found inconsistent mental health results in our three recent studies examining the impact of aircraft noise on child health around Heathrow airport. In the West London Schools Study, aircraft noise was weakly associated with hyperactivity and psychological morbidity as measured by the Strengths and Difficulties Questionnaire (SDQ) completed by parents. The SDQ is one of the most widely used psychometric tests screening to detect psychological morbidity in children in both the UK and internationally. However, in our other two studies using both the parent completed SDQ, the teacher completed Student Behaviour Checklist, and the Child self reported Depression (Child Depression Inventory, CDI) and Anxiety (Revised Child Manifest Anxiety Scale) we did not find any association between mental ill health and aircraft noise exposure.

The Austrian results should be placed within the context of existing studies with respect to two points: (1) the construct being measured in the Austrian study; and (2) the small effect size and inconsistency with previous reports.

In the Heathrow studies we used internationally recognised child mental health screening tools, that have equivalent psychometric properties to those used in the UK (only used in German speaking countries). It is worth noting that the KINDL is normally defined as a “valid and reliable index of quality of life”, rather than a sensitive screening tool to detect specific mental health problems. It is possible that the mental health results reported by Lercher and colleagues are tapping into impaired quality of life and wellbeing, rather than a precise mental health outcome such as “depression”. The definition of “mental health” used by the authors needs to be clarified. The fact that the Austrian results do not replicate our Heathrow results raises the question: Does the KINDL measure wellbeing and quality of life rather than mental health? Furthermore, teacher reports of classroom ability of a male live birth in Ramsar is equal to the period 20 March 1989 to 19 March 2001, (Iranian calendar 1368 to 1379 Hejirae, 103 years), analysis was carried out on the 11 area of Ramsar (currently about 670 per annum), analysis was carried out on the 11 number of annual births in the urban area of Ramsar (currently about 670 per annum), analysis was carried out on the 11 year total for male and female live births, for the period 20 March 1989 to 19 March 2001, equal to Iranian calendar 1368 to 1379 Hejirae Shamsi (HS). The data was not available for the 1378 HS (equal to 20 March 1999 to 19 March 2000). To test the null hypothesis that the probability of a male live birth in Ramsar is equal to the control, the 1926 test was conducted. A value of p < 0.05 was considered significant. The sex ratio is expressed as the proportion of total live births that were males.

The sex ratios at birth in the urban area of Tonkabon, the nearest city to Ramsar (about 20 km distance) and the urban areas of Mazandaran province (excluding Ramsar) were used as controls. The overall sex ratios in Ramsar, Tonkabon, and the urban areas of Mazandaran province were 0.511 (total live births = 7591), 0.517 (total live births = 14 266), and 0.509 (total live births = 253 918), respectively. There was no significant difference between Ramsar and either Tonkabon (χ² = 0.95, df = 1, p = 0.33) or urban areas of Mazandaran province (χ² = 0.13, df = 1, p = 0.71).

It has been reported that the sex ratio in the offspring of male radiologists is significantly lower than that in control populations. However, this is not consistent with the present result. This discrepancy could be attributed to the exposure of both parents to ionising radiation. Alternatively, because the inhabitants of Ramsar have lived for many generations in an area of high background radiation,
some kind of adaptation might have occurred. This study was supported by Shiraz University

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References

William Harvey and air pollution
Thomas Parr died, on 14 November 1635, at what was recorded as the advanced age of 152 years and 9 months. A postmortem examination was performed and a record made by Harvey of the cause of death. The nature of the disease was included in the Everyman edition of De Motu Cordis. 1 Parr seemed remarkably well preserved, and when considering the cause of death, Harvey identified air pollution as a possible contributory factor. His words are worth reading:

"It was consistent to attribute the cause of death to sudden adoption of a mode of living unnatural to him. [Parr had been brought to London not long before he died by Lord Arundel.] Especially did he suffer harm from the change of air, for all his life he had enjoyed absolutely clean, rarefied, coolish, and circulating air, and therefore his diaphragm and lungs could be inflated and deflated and refreshed more freely. But life in London in particular lacks this advantage—the more so because it is full of the filth of men, animals, sewers, and other forms of squalor, in addition to which there is the not inconsiderable grime from the smoke of sulphurous coal constantly used for fuel for fires. The air in London therefore is always heavy, and in autumn particularly so, especially to a man coming from the sunny and healthy districts of Shropshire, and it could not but be particularly harmful to one who was now an enfeebled old man." 

Harvey went on to point to the possible adverse effects of changing from a simple diet to a rich one. Harvey’s observation on the possible effects of air pollution are interesting in that they antedate Evelyn’s much better known analysis by 26 years. In retrospect we can see that Harvey identified the effects of short term exposure to high levels of air pollution on a vulnerable person.

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Alternative methods of administering amyl nitrite to victims of cyanide poisoning
The traditional method of administering amyl nitrite to a victim of cyanide poisoning, is to break an ampoule in a handkerchief and then intermittently hold this under the victim’s nose. 3

I would like to suggest two alternative methods for administering amyl nitrite. The first method is to use a nebuliser. The second method is to use an inhaler similar to the Penthrox device, normally used to administer methoxyflurane for emergency anaesthesia. With appropriate training, either method could be used by first aid staff. This could be of particular value to remote mine sites where the absence of medical staff may preclude intravenous administration of cyanide antidotes such as dicyclo edate, sodium thiosulphate, sodium nitrite, or hydroxocobalamin.

Both methods offer the following advantages over the traditional method:
• Oxygen can be administered during treatment
• Rapid delivery of the drug
• Accurate dose delivery
• Less risk of inhalation by first aid or medical staff
• Less risk of injury due to glass fragments.

The inhaler device would also be particularly well suited to the treatment of large numbers of victims following industrial disaster or terrorist attack—the risk of which has been recently alluded to. 4

One concern about introducing these methods is the potential for amyl nitrite toxicity. Experimental research is recommended to determine safe dosages and frequencies for each method.

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References

Basic Statistics and Epidemiology, A Practical Guide

This book is “aimed at people who want to understand the main points, with minimum fuss”—no small task when the subject at hand is statistics! However, this book manages it by using short, concise, easy to read chapters that contain simple examples and a minimum of mathematics. The style is suitable both as a text to read from start to finish and as a reference book. It introduces students to the basic terms and concepts in statistics and epidemiology and provides a very basic “walk through” of some simple formulae.

The book is loosely divided into two parts. It begins with a brief description of what are statistics, their role in the study of populations, and ways in which samples can be drawn from populations in order to make statements about individuals in the population. Concepts such as probability, significance testing, and standard errors are introduced and explained before a very brief mention of some simple statistical tests. In these later chapters insufficient information is provided to allow the reader to understand the mechanisms of these tests, or the conditions required for their application. However, useful references are given where the reader may find further detail.

In the second “half” of the book the author covers basic epidemiological concepts, describing the difference between prevalence and incidence, and how to measure disease frequency, and discussing bias and confounding. Later chapters in this section present basic study designs such as cohort, case-control, and randomised clinical trial (or RCT), and describe the planning and use of questionnaires.

The book provides a useful glossary of terms, including mathematical symbols and a number of statistical tables. A set of exercises is given and answers are provided. These are invaluable addition to the book.

For the non-mathematical health student faced with the daunting prospect of having to begin studying statistics, this 150 page book is an excellent primer. It introduces basic terms and concepts and gets the student started. However, statistical concepts can be difficult to understand, and in some chapters in this book the brief introduction given falls short of helping the student understand material properly. Therefore the interested student may see this book as a first introductory text, shortly to be followed or indeed accompanied by a more full statistical or epidemiological textbook. For this purpose an excellent, current bibliography is provided.

R Atkinson

Occupational Disorders of the Lung: Recognition, Management and Prevention

The authors of this book aim to draw attention to “the changing nature of the contribution the occupational environment makes to lung disease, and to the particular difficulties this poses for those who find themselves responsible for patient care or the management of relevant industries”. The result is a book which is easy to read and helps greatly by use of a standard format for each chapter. The format includes management of both the individual and the workforce, and prevention. The authors have also used difficult or “grey” cases, similar to one other textbook in the field. The difference here is

www.occenvmed.com
Bone’s Atlas of Pulmonary and Critical Care Medicine, 2nd edition

Edited by G Douglas Campbell Jr and D Keith Payne (pp 315 plus index and colour plates; $55) 2001. Hagerstown, MD: Lippincott Williams & Wilkins. ISBN 0 7817 3436 3

This book aims to cover an enormous subject, and the editors have done very well to contain it to a little over 300 pages. Its 26 chapters are grouped into six sections, the lion’s share being occupied by respiratory topics, with critical care being limited to the relatively short final section. The atlas format is ideal for busy clinicians and students of chest disease absorb a large amount of information in a relatively short amount of time.

Despite the numerous contributors, the book’s layout is uniform and very accessible; text is limited and punchy and extensive use has been made of diagrams, flow charts, and tables to supplement the generally good quality photographic images. The grouping of the colour plates to the final pages of the book, to contain printing costs, is a little distracting but a justifiable compromise.

All of the material is up to date and well referenced, though tends to some extent to be dominated by North American sources and opinion. I found the chapters dealing with lung cancer, bronchiolitis obliterans and other bronchiolar airway disorders, and sarcoidosis to be particularly useful and excellent sources of a large and diverse amount of information. In contrast the chapter dealing with interstitial lung disease was to me a little disappointing. The chapters covering sleep disorders, HIV and fungal infections, lower respiratory tract infections, and nutrition are new to this edition and are welcome additions. The use of graded evidence based recommendations for diagnostic and therapeutic interventions is variable between chapters and its more consistent application would add further to this book’s already considerable value.

I am sure this atlas will have broad appeal to both undergraduate and postgraduate students of chest medicine as well as busy practitioners. It would be a valuable aid to those preparing for postgraduate exams as well as specialist registrars in respiratory medicine, who I’m sure would find it a very useful source throughout their trainee years. Intensivists and trainees in critical care will, I expect, find the balance towards respiratory medicine less appealing. It has few competitors in terms of its breadth and clarity and it represents good value for money; in short it deserves a place in all good medical libraries.

W S Tunnicliffe

The Health Effects of Chrysotile Asbestos


The famous mortality study led by Corbett McDonald has followed 11 000 Canadian chrysotile miners and millers until 80% were dead; only 33 mesotheliomas were reported and excess lung cancers occurred only at high exposure levels. Yet that same chrysotile used in textile manufacture in South Carolina was associated with a 50 times greater lung cancer mortality.

This volume, published in 2001 by The Canadian Mineralogist, reports the papers presented and the ensuing discussion and commentary at a symposium in 1997 called by the Canadian Government to discuss the health issues surrounding the continued production and use of chrysotile asbestos. Can the mineral be used safely? To most uninformed observers, the answer must be a clear no. The true answer is of course not so clear cut. Much of the evidence suggests that chrysotile itself is much less hazardous than the amphiboles and that the serious risks associated with chrysotile are a consequence of its contamination by tremolite, an amphibole that is found in geological intrusions into the chrysotile ore body. These are the issues discussed by the distinguished geologists, mineralogists, epidemiologists, risk analysts, and pathologists who contributed to the symposium. Among them are the last published contributions of two who made great contributions over many decades to investigating the hazards of asbestos and to protecting workers, the late Chris Wagner and Bob Murray.

The resolution of this conundrum may seem unimportant to those who live in countries where past exposures have been to mixtures of amphiboles and chrysotile and where use of asbestos has effectively ceased. However, industry continues to need durable fibres and the poor world sees substantial advantages in using cheap asbestos cement for water pipes and roofing material. And the issue is of course important to the Canadian and Russian chrysotile industries and their employees. Anyone who has been involved in the asbestos debate, who gives advice to industry or lawyers on asbestos issues, or who is interested in the complexities of the interface between science and regulation will find much of fascination in this volume.

J G Ayres
NIVA Training Programme 2003: Advanced Courses in Occupational Health and Safety

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Introduction to Occupational Epidemiology
23–29 October 2003, Hotel Gentofte (Copenhagen), Denmark

Work-related Musculoskeletal Disorders: Current Research Trends
1–7 November 2003, The Sunborn Yacht Hotel, Naantali, Finland

CORRECTIONS

The authors of “Association between job strain and prevalence of hypertension: a cross sectional analysis in a Japanese working population with a wide range of occupations: the Jichi Medical School Cohort Study” (Tsutsumi A, Kayaba K, Tsutsumi K, Igarashi M, Occup Environ Med 2001;58:367–7) have asked for the following errors to be pointed out.

• There are errors in the abstract (line 16) and text (page 368, left hand column, line 5). A part of the baseline data was collected in 1995 in two of the 12 study sites so that the correct period was 1992–95 (not 1992–94).
• On page 368, left hand column, line 24, the number of older participants (over 69) should be 696 and not 699. These facts do not, however, affect the study findings.

We apologise for the following error in the book review, “Late lessons from early warnings: the Precautionary Principle 1896–2000” by R L Maynard. A copy of this book is available to download free of charge from EEA Online. The URL, however, was published incorrectly. The correct link is: http://reports.eea.eu.int/environmental_issue_report_2001_22/en.
Will sewage workers with endotoxin related symptoms have the benefit of reduced lung cancer?

J H Lange, G Mastrangelo and K W Thomulka

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